NBER WORKING PAPER SERIES

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Working Paper 19153
http://www.nber.org/papers/w19153

NATIONAL BUREAU OF ECONOMIC RESEARCH
1050 Massachusetts Avenue
Cambridge, MA 02138
June 2013, Revised November 2017

We thank Sonia Bhalotra, Pascaline Dupas, Lena Edlund, Monica Das Gupta, Richard Freeman, Supreet Kaur, Suresh Naidu, Christian Pop-Eleches, Martin Ravallion, Rodrigo Soares, and Miguel Urquiola for helpful comments. We thank James Heckman and anonymous referees for guidance and suggestions that improved the paper. Matthew Turner provided data on the 1980 rail network and Yi Cheng, Lucy Lu, Jie Sun, and Yixin Sun research assistance. Almond was supported by NSF CAREER award #0847329. The views expressed herein are those of the authors and do not necessarily reflect the views of the National Bureau of Economic Research.

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JEL No. I15,I25,I32,J13,K11,N35,P26,Q18

ABSTRACT

Following the death of Mao in 1976, agrarian decision-making shifted from the collective to individual households, unleashing rapid growth in farm output and unprecedented reductions in poverty. In new data on reform timing in 914 counties, we find an immediate trend break in the fraction of male children following rural land reform. Among second births that followed a firstborn girl, sex ratios increased from 1.1 to 1.3 boys per girl in the four years following reform. Larger increases are found among families with more education and in counties with larger output gains due to reform. Proximately, increased sex selection was achieved in part through prenatal ultrasounds obtained in provincial capitals. The land reform estimate is robust to controlling for the county-level rollout of the One Child Policy. Overall, we estimate land reform accounted for roughly half of the increase in sex ratios in rural China from 1978-86, or about 1 million missing girls.

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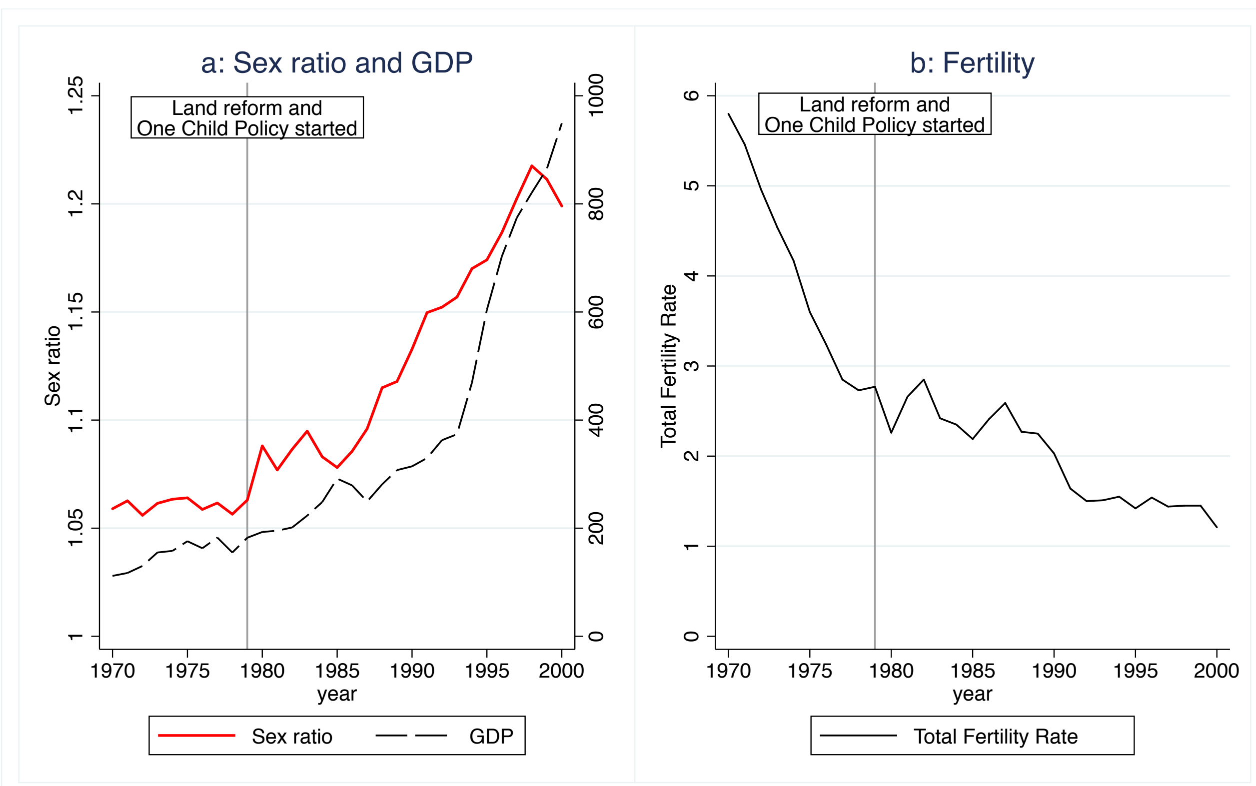
An online appendix is available at http://www.nber.org/data-appendix/w19153

1 Introduction

Economic development has narrowed gender gaps over the past quarter century, including those in educational attainment, life expectancy, and labor force participation (WorldBank, 2012). Nevertheless, perhaps the starkest manifestation of gender inequality – the "missing women" phenomenon – can persist with development (DasGupta et al., 2003; Duflo, 2012). Figure 1a shows its evolution in China. Despite rapid GDP growth since 1980, the sex ratio at birth increased from 1.06 in 1978 to 1.20 in 2000. In 2010, the sex ratio remained 1.19, or about 500,000 more male births per year than the biological norm of around 1.05 males per female. Previous research on this perverse effect of economic development has focused on two mechanisms: reductions in the cost of sex selection (e.g. ultrasound diffusion in Chen et al., 2013) and reduced fertility (e.g. Jayachandran (2017)). Surprisingly, China's sex ratios started to rise in 1980, when costs of sex selection were high, and moreover when fertility was relatively flat (Figure 1b). Why?

Figure 1: Sex ratios, GDP and Fertility in China: 1970-2000



Notes: GDP per capita (current US$) data are from World Bank. Sex ratios are aggregated from microdata of the 1982, 1990 and 2000 Census. Data on total fertility rate are from Cai (2008).

We propose a new factor affecting sex selection: 1978-84 land reform in rural China (then home to 86% of the Chinese population). Introduction of the "Household Responsibility

System" unraveled collectivized agriculture and marked a critical first step toward a market–based economy. The reform granted land user-ship rights to individual households on a long-term basis, while land ownership remained with the collective. It is well documented that land reform spurred remarkable growth in agricultural output (McMillan et al., 1989; Lin, 1992) and lifted *hundreds of millions* of rural households out of poverty (WorldBank, 2000).[1] The main empirical challenge is to disentangle the effect of land reform from that of the One Child Policy (OCP), which started around the same time.

We analyze new data from primary sources, including county records on land reform adoption in 914 counties, covering half of the rural population, and merge these with the 1990 population Census microdata. Previous research focused on the rollout to 28 provinces (Lin, 1992). We also collect previously unanalyzed data on the county-by-county rollout of the OCP during 1979-83. There is substantial variation in the timing of both land reform and the OCP at the county level, which enables us to evaluate both policies and capture their interactions. Our focus on the local rollout of signature national policies parallels recent work on US counties, e.g., Isen et al. (2017); Hoynes et al. (2016).

Our empirical strategy is based on a demographic regularity: the sex ratio of the first child is biologically normal but becomes abnormally male-biased at higher birth orders, especially among Chinese families with no previous son (Zeng et al., 1993). We find a clear increase in the fraction male for second children in families without a firstborn son. Prior to land reform, we do not see trends in this sex ratio. Nor do we see substantial increases in sex ratios following land reform for either the firstborn child or the second child if the first child was male. These raw patterns are replicated in our regression estimates that remove unrestricted county by year fixed effects. We find a precisely estimated trend break of 0.7 percentage points per year in the fraction of males following a first girl after land reform, which accumulates to a 2.8 percentage point increase four years post reform (isomorphic to the raw sex ratio rising from 1.1 pre-reform to 1.3 post-reform).

We provide empirical evidence on potential confounders. These must mimic the rollout of land reform by county *and* differentially affect families with the first child being a daughter versus a first son. From our systematic review of reform policies, candidate confounders do not adhere to this precisely-prescribed pattern with one potential exception: the OCP. We find robust results for land reform. Estimates on the land reform effect are undiminished when the county-level OCP rollout is controlled for. Second, land reform increased sex ratios before the introduction of the rural OCP. We also find that the number of second births is not affected by land reform conditional on the OCP, reducing concerns about potential changes in the composition of families caused by land reform.

[1] Using time-series data, McMillan et al. (1989) suggest that over three-quarters of the productivity increase in 1978-84 could be attributed to land reform. Using provincial reform rollout, Lin (1992) finds that the reform accounts for half of realized output growth.

There is no shortage of explanations for why land reform may have increased sex selection. *A priori*, we view two mechanisms as particularly plausible: increased income (wage and/or non-labor income of parents) and a greater productivity benefit of sons. We formalize these mechanisms in Appendix A.[2] Empirically, we find more consistency with the income mechanism. The sex selection response was concentrated among parents with more education and in counties with higher income growth after reform. Within the subset of "productive son" models, those that reward a second son would predict sex selection for first births as well as for second births with an elder brother. However, these predictions are not supported by the empirical evidence. Moreover, in areas/crops where males are more productive, "productive son" models predict a larger sex selection response to land reform. Using previously unanalyzed occupation and industry codes from the 1982 Census, we do not find this predicted heterogeneity. We discuss five additional hypotheses and likewise do not find supporting evidence. That said, we are more circumspect in interpreting empirical evidence on mechanisms than our reduced-form because our mechanism measures are relatively crude.

Finally, how did land reform increase sex selection? Parents might prefer to conceal sex selection behaviors, and as such detecting them is "forensic economics" (Zitzewitz, 2012). Like previous studies, we unfortunately do not have a direct measure of sex selection. But sex ratios have a natural benchmark (1.05) and ultrasound technology was introduced in China's provincial capitals during the 1970s, permitting non-invasive prenatal sex determination. Combining data on ultrasound machine diffusion to provincial capitals collected by Chen et al. (2013) with the 1980 rail network data provided by Matthew Turner, we find larger increases in sex ratios in rural counties with railroad connections to provincial capitals where ultrasound machines were available. We also find increased sex selection due to land reform in places where ultrasound was unavailable, possibly through greater postnatal selection. A common feature of sex selection in both contexts is that parents find it costly.

The role of land reform in sex selection has gone unrecognized for more than thirty years, we suspect, because county-level data on land reform timing were unavailable. Effects of China's economic watershed are interesting *per se* and suggest a perverse effect of economic development. The persistence of sex selection among Asians in the West (Dubuc and Coleman, 2007; Almond and Edlund, 2008) underscores that factors beyond parochial ones like the One Child Policy or physical brawn help account for "missing girls".

[2] We consider the response of sex selection and fertility to either increased income or the increased opportunity cost of childrearing (wage) from land reform. A son increases parental utility, but may also increase wages or income, i.e. a productivity channel.

2 Background

2.1 The post-Mao land reform

From 1956 to 1977, China was a planned economy. In agriculture, prices were centrally controlled and fixed, trading in the market prohibited, and production was organized collectively in production teams, making it difficult to monitor and reward individual effort. Unsurprisingly, China's grain output per capita stagnated during this period (Zweig, 1987).

In 1978, two years after the death of Mao Zedong, the Chinese government initiated some fledgling, "top down" reforms in an attempt to increase productivity. Party leaders reached consensus on three national interventions: raising the long-depressed state procurement prices for major crops, reducing grain procurement quotas, and opening inter-regional trade (Perkins, 1988; Lin, 1992).[3] More structural changes to agricultural production were considered too radical by Mao's designated successor Hua Guofeng.

The more substantive agricultural reform of 1978-84 thus came about as a grassroots movement that broke with official policy. At the end of 1978, a small number of production teams in Anhui Province experimented with contracting land and assigning output quotas to individual households. A year later, these teams harvested yields far larger than other teams (Lin, 1992). As the movement spread, increased agricultural output softened official resistance. The Party's prohibition was relaxed in 1979 by allowing exceptions for poor regions. When Hua Guofeng was replaced by Zhao Ziyang in 1980 and Hu Yaobang in 1981, the reform gained acceptance and was rolled out more quickly. In January 1982, Central Document No.1 officially announced that "the Household Responsibility System (HRS) is the production responsibility system of the socialist economy".

In essence, land reform allowed collectives to allocate an equal share of land to each individual; households could make input decisions and receive all residual income from land after meeting procurement obligations to the state (Perkins, 1988). Land reform created variation across time and space (Lin, 1992), which we will explore in the empirical analysis below.

2.2 The One Child Policy in rural areas

China started the One Child Policy (OCP) in 1979 and financial penalties were introduced to enforce it. While a strict one-child rule has been applied in urban areas since 1979, a second child was allowed in rural areas until 1984. Only a *third* or higher-parity child in rural areas was punished in the 1979-1983 period (Banister, 1987).[4] Fertility control was decentralized

[3]Decentralized price and market reforms did not come until 1985, when the central government announced that mandatory procurement quotas in agriculture were no longer permitted (Sicular, 1988).

[4]The rural OCP was changed in 1984 to allow a second child following a first girl, the so-called "1.5 Child" Policy (Greenhalgh, 1986), which is unlikely a confounder to land reform because it came after land reform

to local governments, and resistance from parents led to a gradual implementation of the policy (Scharping, 2003). Overall in this period, penalties for above-quota births in rural areas were relatively mild. More substantial fines were imposed only later (Ebenstein, 2010).

Fertility was higher following 1979 than commonly believed. As shown by Figure 1b, well before the OCP, the total fertility rate fell by nearly *half* from 1970 to 1977. It "bottomed out" at about 2.5 children around 1979, where it remained through out the early OCP period until 1987. The macro trend suggests that the OCP played at most a modest role in affecting fertility during our study period.

3 Data

3.1 County-level reform rollout

Data on county rollout of land reform and the OCP come from county Gazetteers. Gazetteers are compiled by local historians to record local history and draw upon materials in local archives. They were not used in evaluating local officials and *prima facie,* are less susceptible to misreporting. To empirically gauge the quality of Gazetteer data, we compare economic statistics in Gazetteers to the commonly used statistics in yearbooks, on which cadre evaluation was based. Appendix Section B.1.1 reports results using gross production of grain as an example. First, we document substantial agreement between the two data sources. Second, we show that the Gazetteer measures respond more to rainfall and soil quality than yearbook data. Third, we apply Benford's Law in manner suggested by Varian (1972) to detect fake data, where falsified digits tend to be made up uniformly. To summarize, we find that Gazetteer data are similar to yearbook data but appear more accurate when they disagree.

We conducted a comprehensive review of all county Gazetteers published to date. Our primary analysis sample includes the 914 counties (half of China's rural counties) that record precise timing of both land reform and the OCP. These reporting counties are very similar to other counties (Appendix Table A.5).

The start of land reform is defined by the year when collectively-owned land was first contracted to individual households in a few villages for each county; it usually took 2-3 years for it to spread to the entire county. We find in Appendix Section B.1.2 that the pattern in the county-by-county reform rollout is consistent with both government policies discussed in Section 2.1 and the existing literature on the reform rollout by province.

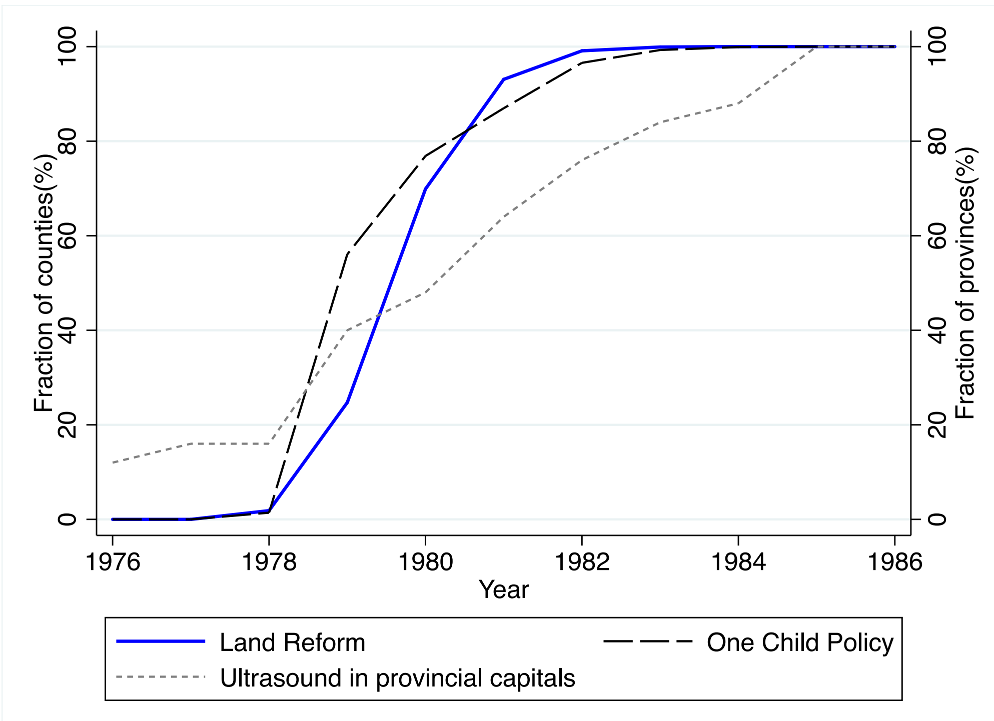
We define the beginning of the OCP as the year when the county government issued the first policy document to enforce penalties for above-quota, third births. The county rollout data correspond to local implementation: we find that fertility reduction following the county

was completed. We discuss the "1.5 Child" Policy in Appendix Section C.4.

policy indeed occurred at the third parity and above (Appendix Table A.4). We also find that fertility responses to the policy rollout scale up to replicate national fertility trends.

In Figure 2, the solid line represents the fraction of counties that started land reform between 1978 and 1984 and the long-dotted line shows the rollout of the OCP, both scaled by the vertical axis on the left. Despite similar timing in aggregate, land reform and the OCP show substantial differences in their county-level rollout. Land reform came earlier than the OCP in 27% of counties, in 25% they coincided, and in 48% the OCP came earlier (Appendix Figure A.3). At the county level, the correlation between HRS timing and OCP timing is -0.005.

Figure 2: Reform rollout



To confirm land reform's previously-documented effect on agricultural productivity, we combine two variables in the Gazetteers to calculate grain output per capita: annual gross production of grain divided by annual population by county. There are 415 counties that report both the reform timing and complete year-by-year grain production and population from the 1970s to the mid-1980s.[5] Because county statistics have only been released systematically in yearbooks since the 1980s in China, Gazetteer data from the 1970s are particularly valuable in assessing the timing of income increases as they relate to land reform.

[5]Appendix Table A.5 also shows that these counties with grain data are comparable to other counties.

3.2 Diffusion of ultrasound technology to provincial capitals

Data on the diffusion of ultrasound are provided by Chen et al. (2013), also collected from county Gazetteers. Just 4% of rural counties had ultrasound machines by 1982, when the rollout of land reform was nearly completed. Although prenatal sex determination was all but unavailable locally, it was accessible in certain provincial capitals. The first ultrasound machine arrived in Xi'an in Shaanxi province in 1965. Other provincial capitals started to acquire their first machines in the late 1970s. In Figure 2, the short-dotted line shows the rollout of ultrasound machines to the 30 provincial capitals (on the secondary vertical axis). During the rollout of land reform, one option for pregnant women (especially for those in rail-connected rural counties) was to travel to the provincial capital to ascertain fetal sex. In Section 7, we examine to what extent sex selection induced by land reform operates through ultrasound access in provincial capitals.

3.3 Sex ratios from census microdata

To consider child sex ratios during the reform period, we use the 1 percent sample of the 1990 Census microdata in 914 counties. We focus on individuals born 1974-86, who were aged 4-16 in 1990.[6] Our main sample includes second children in all families with at least two children.[7] In our study period, 92% of women over age 35 had at least two children.

Because the Chinese census does not explicitly query one's birth order and sex of siblings, we use information on one's relationship to the household head to identify the household head's children and order these children using their month and year of birth.[8] To verify this order is complete, we require that the number of children linked to the household head is equal to the number of surviving births reported.[9] Even for the oldest cohort of second children in our sample, we observe the firstborn child in their family because he/she was still too young to leave home in rural China. Indeed, when we compare the distribution of birth year of first children in the 1990 Census to that in the 1988 National Fertility Survey, in which parents report complete fertility histories, the two distributions are nearly identical (see Appendix Figure A.4).

An advantage of analyzing the 1990 Census is that internal migration was under strict control and was not relaxed until the 1990s (Wang, 2005). Although the Chinese Census

[6] The National Bureau of Statistics reported that the child underreporting rate was 0.7%. While low, child underreporting is more common under age 4 in the census year (Zhang and Zhao, 2006). Therefore, we focus on children older than 4. We also check the robustness of estimates by including children under age 4.

[7] We also check the robustness of estimates using all births at the second parity and above.

[8] Twins and triplets (2.3% of all births) are not analyzed because birth order is more difficult to identify and interpret.

[9] In our sample, 81% of women report the number of surviving births equal to the number of children observed in their family in the census. We also confirmed the robustness of estimates to including families that report a different number of surviving children than that observed in the family.

does not query county of birth, one's county of residence in 1990 maps very closely to county of birth. The migration rate is 0.63% among families with children in our analysis sample.[10] We exclude these migrants in our analysis.

Summary statistics are reported in Appendix Table A.6. The fraction of males among first births is stable at 0.51 before and after land reform. Among second births following a firstborn boy, the fraction of males remains about 0.5 before and after the reform. The most striking change is observed among second births following a firstborn girl: the fraction of males increases from 0.53 before land reform to 0.56 after the reform. Similarly among all births at the second parity and above, the fraction of males following no previous son increased by 4 percentage points after the reform, while the fraction is stable following at least one previous son.

4 Empirical Strategy

4.1 Event studies

We start by describing grain output and sex ratios before and after land reform (no regression adjustment). The relatively flat trend of grain output per capita prior to reform (before time 0) in Figure 3 confirms sluggish productivity growth under the collective system. One year after the reform started, it turns sharply to an upward sloping trend. We estimate the change in slope in Appendix Table A.15 (column (1) of Panel A) and find a 3.5 percent increase in grain output per capita per year following reform. The change in slope in agricultural productivity is consistent with the gradual spread of the HRS within county (recalling that our measure of land reform "turns on" when the first villages adopted HRS).[11]

[10] In the 1990 Census, a migrant respondent is defined as not residing in the same county in 1985.

[11] It is also possible that households gradually respond to the incentives under the HRS.

Figure 3: Event study of grain output per capita

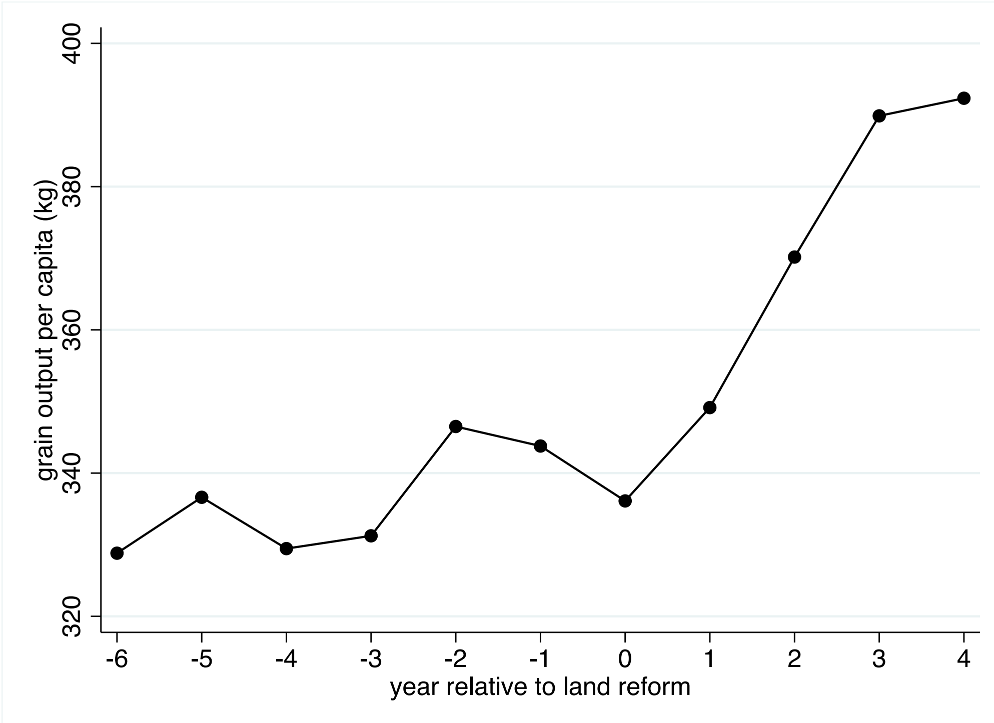


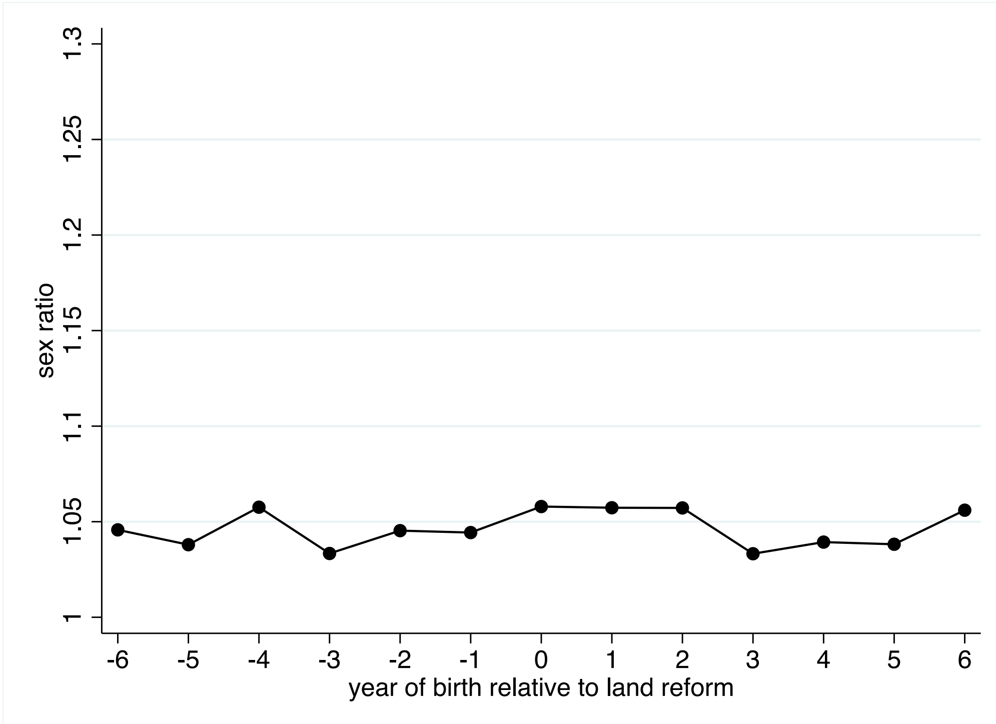
Figure 4a presents sex ratios of first births by year of birth relative to the start of land reform. The sex ratios are stable at the biologically normal rate of 1.05 before and after the reform, suggesting the absence of sex selection among first births.

Figure 4b shows sex ratios of second children in families with a first girl and those with a first boy before and after land reform. Importantly, there are no pre-existing trends of second children's sex ratios in either families with a first boy[12] or families with a first girl. Moreover, among families with a first boy, little change in sex ratios of second births is observed post reform. In stark contrast, sex ratios following a first girl show sharp increases right after the reform, from 1.1 to 1.3 six years post-reform. The slope change in the sex ratio trend following first girl is consistent with the timing of the slope change of grain output in Figure 3: both departures were immediate but took several years to manifest fully.

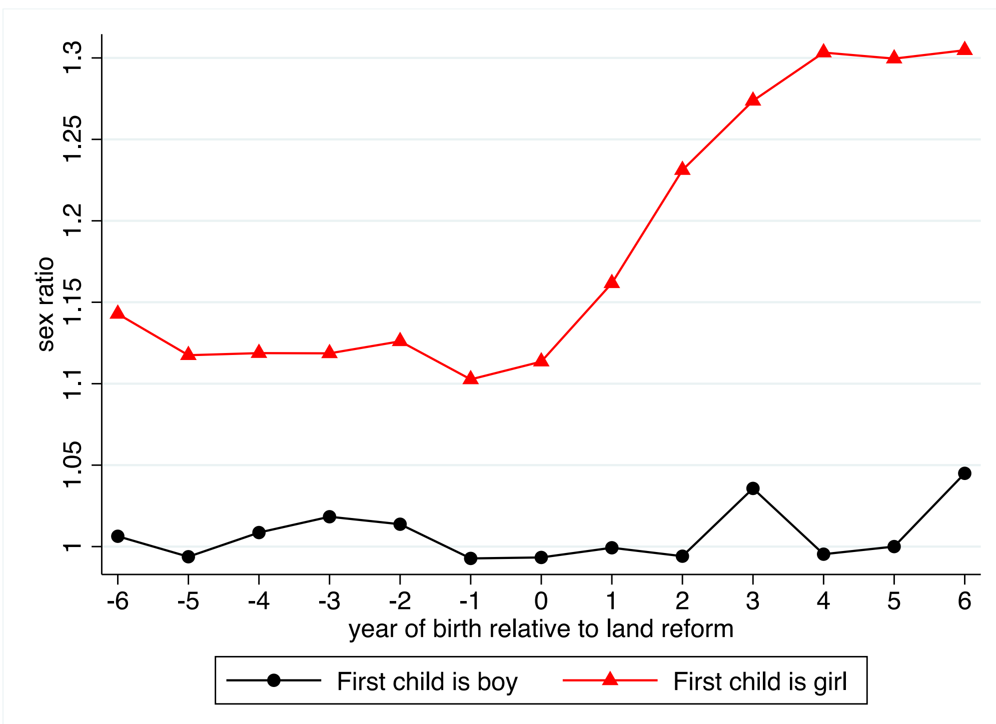
[12] Following a first boy, girls are slightly more common than biological norm, as noted by Chen et al. (2015), who argue that girls were adopted by families with sons.

Figure 4: Event study of sex ratios

(a) First children



(b) Second children



4.2 Econometric specification

To assess the post-reform slope change in sex ratios following a first girl, we estimate a trend break model:

$$\begin{aligned} y_{ijt} = &\alpha + \beta_1 \mathtt{E}_{jt} * \mathtt{GirlFirst}_{ijt} + \beta_2 \mathtt{Reform}_{jt} * \mathtt{E}_{jt} * \mathtt{GirlFirst}_{ijt} \\ &+ \beta_3 \mathtt{GirlFirst}_{ijt} + \gamma_{jt} + \epsilon_{ijt}, \end{aligned} \tag{1}$$

where i is family, j county of birth, and t year of birth. y_{ijt} is a dummy variable equal to 1 if the second child is male in family i. $\mathtt{GirlFirst}_{ijt}$ is a dummy variable equal to 1 if the first birth is a girl. Event time $\mathtt{E}_{jt}$ is defined as birth year minus the reform year and is interacted with $\mathtt{GirlFirst}_{ijt}$. β_1 measures the average pre-reform trend in the fraction of males among second births following a first girl. $\mathtt{Reform}_{jt}$ is a dummy variable equal to 1 if one is born after the reform and is interacted with $\mathtt{E}_{jt} * \mathtt{GirlFirst}_{ijt}$. The coefficient of interest, β_2, measures the average post-reform slope change in the trend of the fraction of males following a first girl. The increase in the fraction of males k years post reform is $\beta_2 * k$. All regressions include $\mathtt{GirlFirst}_{ijt}$, as well as county-by-year fixed effects γ_{jt} to absorb time-varying county characteristics. γ_{jt} also absorbs event time $\mathtt{E}_{jt}$ and the reform indicator $\mathtt{Reform}_{jt}$ main effects. Standard errors are clustered by county.

In addition to the linear trend break model, we also estimate the average reform effect over the entire post-reform period in our sample by:

$$y_{ijt} = \alpha + \beta_4 \mathtt{Reform}_{jt} * \mathtt{GirlFirst}_{ijt} + \beta_5 \mathtt{GirlFirst}_{ijt} + \gamma_{jt} + \epsilon_{ijt}. \tag{2}$$

β_4 measures the average post-reform increase in the fraction of males in families with a first girl, compared to families with a first boy.

Our key identifying assumption is that without land reform the trends in the fraction of male second births would be the same in counties that started earlier and those that started later. The absence of pre-existing trends in Figure 4b supports the assumption. The remaining concern would be about other policies that closely follow the county-level reform rollout *and* have had differential impacts on the sex of the second child depending on the sex of the first one. The most likely candidate is the OCP. In the following empirical exercise, we disentangle the effect of land reform from the OCP.

For second births following a firstborn boy to be a valid control group, sex of the firstborn child should not be affected by land reform.[13] Figure 4a shows that the sex ratio of the firstborns is at the biological level of 1.05. Finally, our analysis sample of second births should not be selected based on our identifying variable. We therefore examine whether land reform affects fertility at the second parity differentially by the sex of the first child below.

[13] As predicted by the sex selection model described in Section 6.

5 Main results

5.1 Land reform and sex ratios

Table 1 reports trend break estimates in Panel A and average effect estimates in Panel B. Regression-adjusted estimates yield the same results as the raw data displayed in Figures 4a and 4b.

We first examine whether the sex of the first child is affected by land reforms. In column (1), the estimated trend break (Land reform*Event time) in Panel A and the estimated average effect (Land reform) in Panel B are quite small and not statistically significant. Beyond the absence of sex selection for the first births following land reform, these findings also suggest that a biological channel through nutritional improvement is less likely.[14]

Table 1: Land Reform and Male Births

	Male			
	First Child	Second Child		
	All	All	High education mothers	Low education mothers
	(1)	(2)	(3)	(4)
Panel A: Trend break				
Land reform*Event time	-0.001			
	(0.001)			
Land reform*Event time*Girl first		0.007***	0.010***	0.004
		(0.002)	(0.003)	(0.004)
Observations	325,633	241,547	125,601	115,943
R^2	0.006	0.051	0.085	0.097
Panel B: Average effect				
Land reform	0.001			
	(0.005)			
Land reform*Girl first		0.030***	0.038***	0.022***
		(0.004)	(0.007)	(0.007)
Observations	325,633	241,547	125,601	115,946
R^2	0.006	0.051	0.085	0.097
County-by-year FE		Y	Y	Y
County FE and YOB FE	Y			
County-specific linear trends	Y			

Notes: High education mothers completed elementary school. Trend break regressions on first children control for Event time, and on second children Event time*Girl first. All regressions on second children also control for Girl first. Robust standard errors clustered at the county level are reported in parentheses. * significant at 10% level; ** significant at 5% level; *** significant at 1% level.

[14] In particular, improved nutrition after land reform would disproportionately reduce mortality among "fragile males" (Kraemer, 2000) but does not appear to be a major mechanism through which land reform affected sex ratios.

In contrast, the probability of the second child being male increases with land reform if the first child is a girl. For second births in column (2), the trend break (Land reform*Event time*Girl first) in Panel A and the average effect (Land reform*Girl first) in Panel B are both precisely estimated. The estimated trend break is a 0.7 percentage points increase per year in the fraction of males following a first girl after land reform. For example, four years after the reform, 2.8 (0.7*4) percentage points more parents with a first daughter engaged in successful sex selection of their second child than would have occurred had the pre-reform sex ratio trend (flat) persisted. Again, this is highly consistent with the unadjusted event study in Figure 4b. Correspondingly, we find in Panel B that the average effect of land reform in the post-reform period is 3 percentage points.[15]

If land reform affects sex selection through the income mechanism, we would expect parents with a larger income increase after land reform to be more likely to have male second births. Absent an income measure from the Census, we use parental education as a proxy; previous research finds that education contributed to higher agricultural productivity and profits after land reform (Yang and An, 2002). Indeed, we find a large and highly significant trend break among second births whose mothers have more education in column (3): a one percentage point increase per year after the reform. Among less educated mothers, the estimated trend break is smaller and not statistically significant in column (4). Similarly for the average reform effect, mothers with high education engaged in more sex selection after the reform.[16] The heterogeneous response to land reform by parental education suggests that increased parental income is a plausible mechanism through which land reform increased sex ratios.[17,18]

Our estimates are robust to including all births at the second parity or above. We compare all 2+ births following sister(s) versus those following at least one son before and after land reform in Appendix Table A.11. The estimated effect is slightly larger for both the trend break (0.8 percentage points) and average effect (3.8 percentage points) models than that from second births, consistent with greater sex selection at 3+ parity following no previous son seen in cross-sectional analyses (Zeng et al., 1993).

To estimate the contribution of land reform to overall sex ratios, we calculate a weighted

[15] Appendix Table A.7 shows that estimates are robust to including children under age 4. Appendix Table A.8 shows that estimates are robust to including families that report a different number of surviving children than that observed in the family.

[16] Similarly, Appendix Table A.9 shows that children of high-education fathers were more likely to be sons after land reform.

[17] High-education parents have slightly weaker stated son preference than low-education parents: see Appendix Table A.10 using the China In-depth Fertility Survey, Phase I in 1985 and Phase II 1987. Greater sex selection among high-education parents after land reform cannot readily be explained by differences in preferences.

[18] In addition to income, education could affect sex selection through an information channel (better information-seeking on selection methods among the better educated). Access to sex selection technology conditional on income and the information set might be the third channel.

average effect across different parities using our estimates above. The weighted average effect of land reform on overall sex ratios is 3.8%.[19] Given that the overall sex ratios in our sample increased by 6.6% (from 1.06 in 1978 to 1.13 in 1986), land reform contributed to about 58% of the increase in rural sex ratios in this period.

5.2 Land reform *vs.* the One Child Policy on sex ratios

Differences in start dates by county allow us to disentangle the effect of land reform from that of the OCP (Figure 2), which would not be possible in province-level or national data. In this subsection, we focus on whether the county-level rollout of the OCP confounds the effect of land reform on sex ratios.

First, we run a "horse race" between land reform and the OCP in Table 2. For the trend break model in column (1), we include two triple interaction terms as independent variables, "Land reform*Event time*Girl first" and "OCP*Event time*Girl first", where the OCP indicator is equal to 1 if one is born after the OCP was introduced. We find that arrival of the OCP does not generate a trend break as the coefficient estimate is small and insignificant. In contrast, the estimated trend break after land reform is robust to controlling for the OCP effect. In column (2), the estimated average effect of land reform is very close to that in Table 1. Again, we fail to find an average effect of the OCP on sex ratios. These findings suggest that the county-level rollout of the OCP does not confound the effect of land reform.[20]

[19] The estimated average reform effect among 2+ births, 3.8 percentage points increase, implies a 16.5% increase in sex ratios from the pre-reform level. The weighted average effect is calculated as follows: 0*0.44+0*0.56*0.59+0.165*0.56*0.41=0.038.

[20] Nor did the 1.5 Child Policy introduced after 1984 confound the effect of land reform (see Appendix Table A.12). We do find that the 1.5 Child Policy could be an additional factor contributing to the increase in sex ratios since the late 1980s.

Table 2: Land Reform versus the One Child Policy

	Male					
	Trend break	Average effect				
	All	All	pre-OCP	post-OCP	pre-HRS	post-HRS
	(1)	(2)	(3)	(4)	(5)	(6)
Land reform*Event time*Girl first	0.008**					
	(0.003)					
OCP*Event time*Girl first	-0.001					
	(0.003)					
Land reform*Girl first		0.033***	0.025**	0.039***		
		(0.008)	(0.013)	(0.010)		
OCP*Girl first		-0.004			-0.009	0.004
		(0.008)			(0.010)	(0.013)
Observations	241,547	241,547	113,174	128,373	117,991	123,556
R^2	0.051	0.051	0.054	0.048	0.053	0.048
County-by-year FE	Y	Y	Y	Y	Y	Y

Notes: Robust standard errors clustered at the county level are reported in parentheses. * significant at 10% level; ** significant at 5% level; *** significant at 1% level.

Second, we also stratify the sample according to the timing of the two policies. We estimate the average reform effect rather than a trend break because of the short pre- or post-reform period in these exercises. Estimates in columns (3) and (4) suggest that land reform has a positive and significant effect on the probability of second child being male following a first girl for both pre- and post-OCP adoption periods. Thus, land reform has an independent effect.[21] Interestingly, when stratifying the sample by the timing of land reform in columns (5) and (6), we find the OCP has no effect in both the pre- and post-land reform periods. These findings further support that it is land reform rather than the OCP that affects sex selection among second births that follow an elder sister.

5.3 Fertility

In this section, we examine whether land reform affects fertility and thus poses a sample selection issue. Overall, we find land reform has little effect on fertility, while the OCP only has a modest effect (in the expected direction). In column (1) of Table 3, we estimate the effect of land reform versus the OCP on overall fertility: the total number of births by county and year. The estimate of land reform is very small and insignificant.

Fertility decreased by 3 percent after the OCP, and this estimated effect from the county-level rollout can be scaled up to replicate the national fertility trend in Figure 1B: both

[21] In Appendix Table A.13, we test the interactive effect of land reform and the OCP by including their interaction term. We do not find a statistically significant interactive effect, although our confidence intervals are relatively wide and include some large positive interactive effects.

are modest.[22] For second births in column (2), neither land reform nor the OCP had a statistically significant effect. The absence of an OCP effect is consistent with the rural policy: a second child was allowed and financial penalties were on 3+ parity children in the early to mid-1980s. Indeed, we find in Appendix Table A.4 that the decline in overall fertility following the OCP in column (1) is mainly from fertility reduction at the third parity and above.

Table 3: Fertility: number of births by county and year

	ln(number of births)		
	All births	Second births	
	(1)	(2)	(3)
Land reform	0.008	-0.023	
	(0.015)	(0.024)	
OCP	-0.030**	-0.022	
	(0.015)	(0.026)	
Land reform*Girl first			-0.027
			(0.036)
OCP*Girl first			0.173***
			(0.038)
Observations	11,864	11,649	22,532
R^2	0.939	0.833	0.900
County FE and year FE	Y	Y	
County specific linear trends	Y	Y	
County-by-year FE			Y

Notes: Robust standard errors clustered at the county level are reported in parentheses. * significant at 10% level; ** significant at 5% level; *** significant at 1% level.

Turning to the fertility margin most relevant to our identification, we examine whether land reform had differential fertility effects depending on the sex of the first child. To implement, we change the unit of observation in column (3) to county by year by sex of the first child. The estimate of "Land reform*Girl first" is small and insignificant, suggesting that land reform does not affect the number of second births differentially for first girl families and first boy families. Therefore, selected second births may not be a major issue for our main results on land reform.

[22]In Appendix Table A.3, we estimate the change in the national total fertility rate (TFR) after 1979. The decrease is 5 percent from the pre-1979 average TFR. Furthermore, using province-by-year TFR, we find a smaller decrease of 3 percent. These point estimates are similar to our estimate using the county-level OCP rollout here, while estimates are less precise using national or provincial data. Fertility effects of the OCP in the early to mid-1980s were small because fines were mild for relatively rich families were not effectively collected from relatively poor families (Scharping, 2003). The level of fines increased significantly since the late 1980s along with more strict enforcement.

In contrast, the OCP has differential fertility effects depending on the sex of the first child. The positive and statistically significant estimate of "OCP*Girl first" indicates that families with a first girl were 17.3 percentage points less likely to stop having a second child than those with a first boy after the OCP. A resulting concern is that the change in composition by the first child's sex as a response to the OCP could affect the estimated land reform effect in Table 2. In Appendix Table A.14, we use an inverse probability weighting approach to test the robustness of our estimates.[23] We find that re-weighted estimates are similar to estimates in Table 2.

To summarize, we do not find evidence that fertility responses would compromise our findings regarding land reform and sex selection. Furthermore, the modest fertility effect we find for the rural OCP (which maps to the national fertility trend, and in this respect difficult to dispute) would imply a minuscule effect of the rural OCP on sex selection through the fertility channel.[24] The minuscule predicted effect on sex selection is confirmed in Table 2.

6 Why did land reform increase sex selection?

It is well known that land reform boosted agricultural output and income. Here we discuss economic motivations behind sex selection as induced by land reform. Empirically, we find the most consistency with an income mechanism.

The income mechanism

We formalize a stylized sex selection model in Appendix A that allows sex selection and fertility to respond to both income and the opportunity cost of childrearing (wage). We assume that having a son provides utility to parents (Edlund, 1999), and a second son provides no benefit beyond that of a daughter. To support this assumption, Appendix Figure A.1 reports the stated preference among rural parents from the China In-depth Fertility Survey (Phase I in 1985 and Phase II 1987): they indeed desire just one son.[25] We also assume

[23] In the sample of second children, we first predict the effect of the OCP on the probability a second child in a first boy family. Because fewer first boy families are in the sample of second births due to the OCP, we use the predicted probability to assign larger weights to first boy families where the OCP effects are larger. We then run weighted regressions analogous to column (1) and (2) of Table 2.

[24] The 3% reduction in overall fertility comes from 12% fertility reduction among 3+ births (see Appendix Table A.4). 67% of families had more than 3+ children prior to land reform. When the number of children decreases from 3 to 2, the probability of having a son naturally decreases by 14%. Not all those with a reduced likelihood of having a son naturally will sex select. The 1990 rural sex ratio of 1.14 implies that roughly 2.3% of all parents selected a male successfully, or about 4.6% of those parents pregnant with girls. Scaling that 1990 incidence up (arbitrarily) by an order of magnitude, assume that one third of parents would sex select in response to reduced natural probability of having a son. This would imply that sex selection increased by (14%*12%*67%)/3 or 0.4% due to OCP-induced fertility reduction.

[25] Among rural parents, 79% want a son next when they have not already had one, which drops to 31% for parents who already have a son (the other 49% want a daughter and 20% no preference).

selecting a son is costly. Travel costs for accessing ultrasound in provincial capitals were non-trivial for rural parents (who were very poor on average at this time).[26] Additionally, sex selection might impose a psychic cost.[27]

In the model, parents maximize utility over having a second child, having a son, and consumption. They decide among: 1) having a second child with sex selection, 2) having a second child without sex selection, and 3) stopping childbearing following one child. We allow for potentially competing wage and income effects in the decision, as land reform may have affected both. The model first delivers unambiguous predictions of neither sex selection for the first child nor the second child following a firstborn boy, which are supported by results presented in Figures 4a, 4b and Table 1. More importantly, it predicts greater sex selection of the second child following a firstborn girl in response to income or wage increases. This prediction is analogous to that for consumption "goods" without close substitutes that tend to be normal (Black et al., 2013). In contrast, the wage has an ambiguous effect on the decision to have a second child in the model, depending on the magnitudes of income *vs.* substitution effects (Appendix Section A.2).

Our main empirical finding that land reform affects sex selection for first girl families is consistent with predictions from the model. The income mechanism could also explain more sex selection among high education parents after land reform. Furthermore, in Appendix Section D.1, we find corresponding heterogeneity in sex selection by the magnitude of post-reform income growth. The positive trend break in the fraction of males was concentrated in counties with fast output growth after reform, while no trend break is found where grain output had little growth.

In sum, our findings are consistent with the most basic price theoretic framework (and intuition) where sex selection is costly.

The "productive son" mechanism

To consider the possibility that higher productivity and wages are expected from male children,[28] we extend our model so that sons increase wages (Appendix Section A.5.3). Clearly, this increases the benefit to having two sons. As wage increases, parents can be more likely to select both the first child's sex and the second child's sex following a firstborn boy, neither of which is observed in the data. It is also generally not true from the "productive son" model

[26] We collected information on train ticket prices and hotel costs in large cities in the 1970s. Two nights of hotel costed on average 9 RMB. Depending on the mileage, train tickets within province could cost from 0.5 to 4 RMB. Average rural income in 1980 was 191 RMB per person per year (Ravallion and Chen, 2007). If one lived within a two-hour train ride (around 70-80 km) to the provincial capital, one such trip could cost about 6% of one's annual income. And multiple trips might be necessary to achieve (and confirm) a son.

[27] The psychological costs could be larger for postnatal sex selection.

[28] Qian (2008) found that increases in female-specific income, as captured by the relative price increase of tea following tea price reform, increased the survival rate of girls.

that parents are more likely to select the sex of the second child after a firstborn girl, unless additional assumptions are imposed.

A distinct prediction of the "productive son" mechanism is more sex selection when the return to male labor is higher. To evaluate this prediction, we compare land reform's effect on sex ratios in counties more suitable for growing male-labor intensive crops versus those more suitable for female-labor intensive crops in Appendix Section D.2. In the 1982 Census microdata, cotton was the most female labor intensive crop, while fruit had been most male labor intensive.[29] To assess gender specific income, we use crop suitability indices based on agro-climate conditions from the FAO Global Agro-Ecological Zones database. We first ascertain that these indices do predict cropping patterns in China (Appendix Table A.16). We then estimate the interactive effect of the suitability index of each crop with "Land reform*Girl first" on the fraction of males. Estimates of these interactions are all very small and statistically insignificant (Appendix Table A.17). There is no pronounced heterogeneity by gendered wages as the "productive son" mechanism would predict.

Other potential mechanisms

In Appendix Section D.3, we examine five additional hypotheses: *i)* if sons received more land than daughters, this would reward sex selection; *ii)* if land reform destroyed the "collective pension system", parents may have been forced to rely more on their sons for old age support; *iii)* if land reform weakened rural healthcare provision, girls may have suffered; *iv)* if land reform weakened the authority of village administration, monitoring and enforcement of any prohibitions on sex selection may have languished; *v)* if land reform loosened travel restrictions, it could be easier for peasants to travel to access ultrasound. We do not find these hypotheses consistent with the empirical evidence. That said, our evidence on these mechanisms is more suggestive than dispositive and invites additional data collection and analysis.

7 How did land reform increase sex selection?

Was sex-selective abortion feasible for rural Chinese in the early 1980s? Land reform generally preceded the arrival of ultrasound machines in rural China, but ultrasound had become increasingly available in provincial capitals since the 1970s.[30] Because railroad was the main means of long-distance transportation at that time, we consider whether a county was connected by railroad to provincial capitals where ultrasound machines were available.

[29] 35% of workers who grew cotton were male, and 69% of workers who grew fruit were male.

[30] In our basic model in Appendix Section A.2, the costs of sex selection via ultrasound access include travel costs to provincial capitals and psychological costs. In Appendix Section A.5.1, we extend the basic model to include son-rearing costs that yield similar predictions.

Using a digitized national map of railroad networks in 1980,[31] we define railroad access by whether a railroad line passed through a rural county. Every county on a railroad line was connected to the capital city of the same province. 36% of rural counties had railroad access. Access to ultrasound is defined as 1 if a county was connected by railroad to the provincial capital that had ultrasound machines available after land reform. Counties that are assigned 0 either had no rail connection or ultrasound machines were not yet available in the (rail-connected) provincial capital (or both).

Results reported in Table 4 confirm the role of ultrasound access in urban hospitals in sex selection. In column (1), the triple interaction term "Land reform*Girl first*Railroad to provincial capital that had ultrasound" has a positive coefficient statistically significant at the 10 percent level, suggesting a larger land reform effect if parents could take the train from their home county to the provincial capital to access ultrasound.[32] In column (2), we consider the effect of ultrasound access including all travel options to provincial capitals. We interact Land reform*Girl first with a binary variable indicating whether ultrasound was available in the provincial capital, which measures the total effect that comes via ultrasound in the province. Comparing estimates in column (1) and (2), we find that the majority of travel to access ultrasound was via railroad.[33] Finally, in column (2), the estimate of the double interaction term "Land reform*Girl first" suggests a non-trivial and precisely estimated reform effect on sex ratios in places and years without access to ultrasound.[34]

[31] The railroad data are generously provided by Matthew Turner and digitized from SinoMaps Press (1982) (Baum-Snow et al., 2012).

[32] A potential concern is that railroad access might also help peasants connect to a larger input or output market or have more exposure to the urban environment. To isolate the effect of ultrasound access from that of rail access, in Appendix Table A.19 we include an additional interaction term "Land reform*Girl first*Railroad to provincial capital". The estimate of this newly included interaction term is small and insignificant, suggesting that absent ultrasound technology in the provincial capital, rail access *per se* does not increase the fraction of males following land reform.

[33] In our sample, 28% of births occurred in counties and years that had railroad to provincial capitals that had ultrasound. In column (1), the increase in sex ratios through this channel after land reform is 0.0067 (0.024*0.28), which is 20.5% of the total effect (0.0067/(0.0067+0.026)). In column (2), 79% of births occurred in counties and years where ultrasound was available in the provincial capital. Therefore, the share through ultrasound including all travel options is 22.9% ((0.012*0.79)/(0.032+0.012*0.79)), only slightly higher than that through railroad.

[34] In Appendix Table A.20, we stratify the sample by a county's distance to the provincial capital. We find that the effect through ultrasound is strongest for counties within 70 kilometers of the provincial capital.

Table 4: Ultrasound access in provincial capitals

	Male	
	(1)	(2)
Land reform*Girl first*Railroad to provincial capital that had ultrasound	0.024*	
	(0.012)	
Land reform*Girl first* Provincial capital had ultrasound		0.012**
		(0.005)
Land reform*Girl first	0.026***	0.032***
	(0.005)	(0.005)
Observations	241,547	241,547
R^2	0.051	0.051
County-by-year FE	Y	Y

Notes: Column (1) also controls for Girl first and Girl first*Railroad to provincial capital that had ultrasound. County-by-year fixed effects absorb the double interaction of Land reform*Railroad to provincial capital with ultrasound. Column (2) likewise controls for Girl first and Girl first*Provincial capital had ultrasound. The double interaction of Land reform*Provincial capital had ultrasound in Column (2) is absorbed by the county-by-year fixed effects. Robust standard errors clustered at the county level are reported in parentheses. * significant at 10% level; ** significant at 5% level; *** significant at 1% level.

In addition to sex-selective abortion, postnatal sex selection could also be at play for our results because our Census data are on surviving children.[35] In Appendix Section E, we conduct additional analysis on postnatal mortality using the 1992 UNICEF Chinese Children Survey that reports childhood deaths. We find suggestive evidence that the mortality rate among male second births following a firstborn girl decreased after the reform, and that female mortality increased. Parents seem to have allocated some of the land reform bounty to boys, which also contributes to the increase in the sex ratio of surviving children.

8 Conclusion and discussion

Policy makers in Asia have attempted to address high sex ratios by prohibiting prenatal sex determination. China began restricting the use of ultrasound for sex determination as early as 1986 and India issued a similar prohibition in 1994.[36] As the persistence of high sex ratios attests, prenatal sex determination technology is difficult to regulate and continues to improve.[37] It is unclear whether bans provide much of a practical obstacle. In our analysis, sex selection increased despite prenatal sex determination being unavailable

[35] Although postnatal selection renders a costly ultrasound unnecessary, the psychological costs of postnatal selection could be large and thereby generate similar predictions regarding an income effect as ultrasound. Moreover, including son-rearing costs also leads to similar predictions.

[36] Six US states adopted bans on sex-selective abortion 2012-2014 (Rebouché, 2015).

[37] "[S]ex determination could turn into an entirely at-home exercise with home testing kits" (Rebouché, 2015). Morain et al. (2013) describe a "new era" in non-invasive prenatal testing.

locally: poor farmers were willing to bear substantial travel costs. Likewise, fertility did not fall substantially and still sex selection increased.

We find land reform contributed to about half of the increase in rural sex ratios from the late 1970s to the mid-1980s, when sex selection began in earnest in China. Land reform's historical role in fomenting sex selection has been overlooked by researchers, we suspect, because it was introduced at the same time as the One Child Policy, about which priors are strong and county-level data heretofore unavailable. We estimate that land reform lead to about 1 million missing girls (between 0.8 and 1.2 million, 95% confidence interval) in this period. In later periods, land reform may continue to contribute as the trend break clearly persists in Figure 1a.

Our findings add to the documented cultural preference for sons (DasGupta, 1987; DasGupta et al., 2003) by showing that this preference may interact with household income: we are unaware of existing sex ratio research estimating household income elasticities. From a basic price theory perspective, such estimates are overdue. Son preference implies boys and girls are imperfect substitutes. Like other costly consumption "goods" without a close substitute, is having a son normal? Depending on the estimate of how much land reform increased income, income elasticities of sex ratios range from 0.088 to 0.181 (Appendix Section F). Using 0.088 and assuming a linear relationship between income and sex ratios, one would project that the sex ratio in 2000 would be 1.2, similar to the actual sex ratio in the 2000 Census as shown in Figure 1a.[38]

We also benchmark our range of land reform elasticities against two other readily-available "back-of-the-envelope" estimates. First, we consider the cross-country income elasticity in four Asian economies where there is son preference: mainland China, India, South Korea and Taiwan in Appendix Section G. For the 1975-1995 period, we find that a one percent increase in GDP per capita is correlated with a 0.089 percent increase in sex ratios. The analysis of cross-country data, while it includes country and year fixed effects, is mainly descriptive given the potential for confounding. At a minimum, the cross-country approach does not cast doubt on a positive income elasticity. Second, we also consider a public policy designed to increase incomes in rural China. Meng (2013) finds the anti-poverty program of 1994-2000 increased rural income by 38%. In Appendix Section G we see this increased the overall sex ratios by 3.1%, which implies an income elasticity of about 0.082. Both estimates are similar to the lower sex ratio elasticity estimate of 0.088.

As incomes continue to rise and the technology of sex selection disseminates and improves, we might expect elevated sex ratios to increase or at least persist, including among Asians in the West (Almond et al., 2013; Almond and Sun, 2017). On the other hand, ambitious efforts at "triggering normative change within the society as a whole" may be feasible and

[38] Rural income has increased by 220% from 1980 to 2000 (Ravallion and Chen, 2007).

have already achieved some success in Korea (Chung and DasGupta, 2007).[39] Given practical challenges to enforcement, banning sex selection may ultimately be more effective in the normative message sent to parents about gender preference.

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